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Art. I.—ON THE INTRODUCTION OF AIR INTO THE VEINS.

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(Concluded from page 57.)

Estimation of the Cases which have occurred in Human Beings.

Excluding the cases which occurred in animals, I may divide those belonging to man into four classes: the first containing the unimportant cases; the second, those in which death did not supervene; the third, containing fatal cases, in which there was no examination; and the fourth, those where there was an examination.

First Class. *Cases to be rejected.*—These are the cases which are built upon mere hearsay. They are four in number, being those attributed to MM. Gräfe, Lodge, Duportail, and Sir Astley Cooper. It seems certain, indeed, that these are not real facts, and that they have been referred to merely through some mistake.

Second Class. *Cases not followed by death.*—Fifteen of the cases above analysed, in which it is mentioned that the patients came to themselves again, seem, at any rate, to prove that they did not die under the influence of the first symptoms. One of these cases belongs to Dr. Mott, two to M. Clémot, one to Mr. Barlow, one to Dr. Warren, one to M. Roux, one to Mr. Mirault, one to M. Rigaud, one to M. Delaporte, one to M. Dubourg, one to M. Malgaigne, one to M. Bégin, one to M. Toulmouche, one to M. Amussat, and one to myself.

These fifteen cases are of very different values. Those of M. Rigaud, of M. Malgaigne, and Dr. Mott, only indicate a wound of the external jugular vein. Those of MM. Amussat and Toulmouche merely relate to the mammary veins. In Mr. Barlow's case, and in one of M. Clemot's, it also seems that neither the axillary nor the internal jugular vein was wounded. There remain, then, only those of MM. Roux, Warren, Bégin, Delaporte, Dubourg, and my own, with one of M. Clémot's, which allow us to regard the fact as possible, considering the region, and the vein wounded. The case taken by M. Gueretin, from M. Mirault's practice, leaves us in doubt as to the vein opened.

Third Class. *Cases followed by death, but without post-mortem examination.*—These are six in number, and are attested by the names of Warren, Clémot, Barlow, Goulard, Klein, and Maugeis.

In M. Clémot's case, the vein opened is not well determined; in M. Barlow's it was probably the internal jugular; in Klein's, it was the thyroid plexus; and a subscapular branch in Dr. Warren's. M. Duplat asserts positively, that it was the axillary in the case which he attributes to M. Goulard. The median vein at the bend of the arm was the only one opened in M. Maugeis's case. This simple abstract sufficiently shows how vague must be our ideas, when such facts are presented to us.

Fourth Class. *Cases followed by death, where there was a post-mortem examination.*—These are seven in number, and belong to MM. Piédagnel,

Dupuytren, Delpech, Castara, Ulrich, Roux, and Putegnat. M. Putegnat's case being given without any particulars, and on the word of a third person, must, I think, be put aside. M. Piédagnel says it was the external jugular vein which was wounded in Bauchêne's patient. He has also informed me, that in Dupuytren's case the tumour was on the right posterior and lateral region of the neck; consequently it is not possible that the internal jugular vein, or the subclavian, could have been wounded. In M. Roux's and M. Delpech's cases, the operation was amputation at the shoulder-joint, and the only veins wounded when the bad symptoms came on, were unconnected with the axillary. M. Saucerotte says, that in M. Castara's patient it was a subscapular vein which had been opened, to the extent of less than a line, which was also the case in Dr. Warren's. Hence M. Ulrich's case is the only one which affects the region where, according to our experiments, the introduction of air into the veins appears possible and dangerous.

If we now take up the question again at a higher point, we are in some sort compelled to admit the following conclusion:—Either the experiments hitherto performed on living animals are incomplete and deceptive, or else these cases concerning the introduction of air into the veins of man are not conclusive.

Looking upon the question, I see, in fact,—First, by direct experiment, that it requires a large quantity of air to kill a dog; that air does not enter the heart spontaneously, excepting by very large apertures in the jugular, subclavian, or axillary vein; and that the right cavities are then always distended by a red frothy matter, evidently formed by the intimate mixture of blood and air: Secondly, in many of the cases in which human beings were concerned, the only veins wounded were the external jugular, or the veins of the breast, shoulder, or face. In the others the aperture in the vein was small, and but little air could enter; and, lastly, cadaveric examination never showed, in any case, the appearances which have been established by direct experiment.

It is not necessary here to refute the language of those who, no doubt through inadvertence, have always kept beyond the points at issue, both in the course of our experiments, and in the discussions at the Academy; and who have always reasoned as if I and others had denied the possibility and the dangers of the entrance of air into the veins. Without seeking for the motives which may have induced some of my colleagues to fall so continually into this unfortunate error, it is sufficient to refer to the bulletin of the Academy, to show the falseness of the supposition. A point which I must take up, as it might otherwise lead to error, relates to the manner in which some have thought to refute the interpretation which I had put upon certain cases. M. Roux, for example, who, in relating several cases, which, though otherwise interesting, were completely foreign to the subject, thought it very strange that I endeavoured to conclude from animals to man; that I accused observers of not having given all the requisite details of their cases; and that while rejecting one explanation, I gave no other; M. Roux, I say, whose discourse seemed to make an impression on the assembly, yet appears to me, I must confess, not to have thrown light upon any point of the question.

To assert that, at the instant of accidents like these, the surgeon has not sufficient presence of mind, and is too disquieted and too grieved, to think of any thing but the danger which threatens his patient, and that he cannot give an exact and circumstantial account of what has happened, and that in the midst of such occurrences it would be unjust to reproach operators with having published cases destitute of sufficient details, or badly drawn up, is, indeed, a sufficient justification of those who have been placed in these unfortunate circumstances; but does it, in the eye of science, improve the facts, or make them more conclusive? Is it not, on the contrary, confessing at the outset that these facts are necessarily incomplete, and that in so arduous a question it is impossible to bestow our complete confidence upon them?

M. Roux, while allowing that the phenomena observed in animals are far from resembling those which have occurred in human beings, exclaims that this proves nothing; for since symptoms offer numerous varieties, even in different individuals of the same species, or of different species, in direct experiments, we may well believe, he says, that matters will not be the same in men as in dogs. I might answer, that I myself mentioned this supposition to the Academy, and proposed to take it into account long before M. Roux had uttered it. * But he must allow, in his turn, that it is only a supposition; and that the differences between what has been said of human beings and what has been seen in animals, are so striking, that until the contrary has been proved it is impossible not to retain some doubts upon the subject. Besides, M. Roux seems to have forgotten that reasoning of this kind would authorise us, at the outside, to presume the fact in question, to regard it as probable, or as very probable, but that it cannot be demonstrated in this manner. But what is the question between us? Not to know if it is *possible*, *presumable*, or even *very probable;* but whether it is *demonstrated*, irresistibly, that in the cases referred to, death took place from the introduction of air into the veins. Thus, we remain in doubt, which, in my opinion, is the real state of the question.

As to reproaching us with not explaining why the patients died, if we do not allow that they died from the introduction of air into the veins, I think that M. Roux must have done this through inadvertence; for if we must always consider those facts as demonstrated or incontestible of which we cannot give any other explanation, or must admit them, because in denying the explanation of a phenomenon we cannot give any other more satisfactory one, it would certainly not be long before science was overwhelmed with an infinity of dangerous errors. Nevertheless, I must say, that all the cases which have occurred in the human species have something strange, and altogether unusual, about them. Though it is true that patients may die of syncope, hemorrhage, alarm, or exhaustion, during great operations, it is also true that in such cases life is extinguished with a different train of symptoms from those which have been related. If we erase from the list the cases of Klein, Duportail, Lodge, Dubourg, Maugeis, and Sir Astley Cooper, which are either quite unimportant, or which may be easily explained without the supposition of air having entered the veins, it is difficult not to return to this way of explaining the others. If we suppose that in the cases related by MM. Rigaud, Clémot, Bégin, Malgaigne, and myself, the gush of a small artery against some organic layers, or the entrance of air into the sinuosities of a *cul-de-sac*, may have produced the hissing, the bubbling, or the sound of emptying a bottle mentioned by authors, it is almost impossible not to admit something more in the cases of MM. Piédagnel, Dupuytren, Castara, Delpech, Ulrich, Barlow, Warren, and Goulard.

In this state of uncertainty, might one not ask whether, at the moment of the operation in human beings, veins *canalised* by the nature of the tumour itself, or by the handling to which they are exposed, may not be (for the moment) in the state which is the natural one of the veins at the apex of the chest in living animals. With the help of this explanation, however, the only cases which can be brought into the class of *very probable* ones are those of MM. Goulard, Piédagnel, Dupuytren, Castara, Delpech, Warren, and Mirault. It appears to me clear that nothing had caused this transformation in the cases of MM. Rigaud, Amussat, Toulmouche, Mott, and Malgaigne.

Can we attribute it, then, to the previous debility of the patients? Every thing tends to show (although our direct experiments still allow some doubts upon this point) that a great loss of blood must make the entrance of air more dangerous. But on analysing the cases, we see that the patients were still strong, with the exception of those belonging to M. Piédagnel, M. Roux, and Klein; that most of them were in perfect health; and that they had not lost more than a few ounces of blood when the symptoms came on.

If practitioners are unwilling to abandon this comparison, our last resource

is to admit, that either physically, or physiologically, or pathologically, the conditions accompanying the entrance of air into the veins are remarkably different in the human species and in animals. These differences may be regarded as possible, when we reflect that air in the veins kills a horse sooner than a dog—that death is more speedy when the animal is held in one position than in another—that by blowing in air from the mouth, we kill with the rapidity of lightning, while it requires a pretty long time to produce the same effect if a syringe is employed.

I am ready to allow, however, that no one of these reasons is conclusive; and nothing of what I have said is sufficient, at present, to place beyond the limits of doubt what has been advanced in favour of the introduction of air into the veins of man.

The most reasonable thing to rely upon, to aid us against all these difficulties, is the want of care with which the cases have been drawn up. There is no doubt, that if writers had had a more accurate knowledge of what had been already done, they would have entered into minute particulars as to what concerns human beings. There is no doubt, too, but that a number of important circumstances have been either omitted, or inaccurately related, or wrongly explained, by those who have published these cases. I say this without blaming observers for it, because in phenomena which rapidly pass away, and are at the same time complex, and difficult to apprehend, persons do not take every thing into account, unless the question implicated in the phenomena has already engaged their attention, and unless they are well acquainted, beforehand, with all the details which it is possible to collect.

Summary.—Meanwhile, to give my candid opinion, I consider the introduction of air into the veins as *probable* in the cases of MM. Bégin, Malgaigne, Mirault, Warren, Barlow, Delaporte, one of M. Clémot's, the first pointed out by M. Roux, and my own. There is no proof that this accident took place in the cases of MM. Toulmouche, Mott, M. Clémot's second and third cases, or in those of MM. Rigaud, Dubourg, Maugeis, and Amussat. It appears to me *extremely probable* in those of Delpech and Ulrich, and *almost certain* in the cases of Dupuytren, Castara, and Goulard. But after all, this can only be given as my personal belief; for I allow that no one of these facts can be exactly compared with the results of direct experiment, and that, in a scientific point of view, there is not one that carries absolute conviction with it.

Thus my readers will see, that at the end of this discussion I find myself brought back, by force, to repeat what I said on the introduction of air into the veins, in 1832 (Méd. Opér. t. ii. p. 194),—"Without denying the possibility of this phenomenon, when the veins form open canals extending to the thorax, I think that new experiments are required to decide the question."

I will add, as I said in 1833, (Anat. Chir. t. ii. pp. 104, 457, 471, 2d edit.) "That if the fact is really so in human beings, we must look for a different physical explanation from the one given by MM. Poiseuille and Bérard; for it is clear that phenomena resembling those which seem to denote the introduction of air into veins, have been observed in other parts besides the neck and the axilla."

The reader will see, at a glance, how much the question has been cleared up by late experiments, if he compares its present state with what I said of it at the first sitting of the Academy. I then summed up in the following manner:—

"The introduction of air into the veins raises several important questions. There are already about twenty instances (I ought to have said thirty, at least), where serious symptoms, which have supervened during surgical operations, have been attributed to it. Of this number there are several, certainly, which are not conclusive. Bichat thought that it was enough to inject a few air-bubbles into the veins of an animal to kill it, and that death then took place through the brain. Nysten showed, on the contrary, that it required a great quantity of air to kill a dog, and that death then took place

by stoppage of the circulation. Nysten's experiments have been since repeated by a host of persons, among others every year by Magendie, and have always been followed by the same results.

"By showing that the large veins of the root of the neck are always in a state of tension, M. Bérard has proved that air may pass through them when they are open; while M. Poiseuille maintains that this is not possible at more than a few centimetres from the first rib. But in most of the cases several of these conditions are wanting. Thus, Dupuytren's patient had a tumour on the shoulder, and twenty-four hours afterwards only a few air-bubbles were found in his heart. In others, so little air seems to have entered, that, to judge of what we see in animals, they could not have died of it. Lastly, in others the operation was performed upon the face, chest, &c.; and physiological experiments do not allow us to grant, that death can occur by the introduction of air into those parts. Hence the question is still open for examination."

THERAPEUTIC MEANS.

Treatment.—In scientific questions there is a rock which we ought to make every effort to avoid; I mean the error of forming an opinion too soon, and wishing to pronounce a positive decision, in spite of the absence of proofs. Although the facts quoted in favour of the introduction of air into the veins are as yet incomplete, it would be wrong to conclude that this accident has never taken place. For my own part, I repeat that, in my opinion, it has happened several times; only I feel that, until more conclusive proofs are alleged, this can only be a personal belief, and that there is nothing at present in science that can change this belief into a general conviction. With this phenomenon before us, we are like magistrates in whose presence there stands a criminal against whom there are no witnesses.

Holding these opinions, I have reflected, like many others, on the means of warding off the dangers attributed to the introduction of air into the veins. These means must be divided into two kinds, the preventive and the curative.

Preventive means.—There is a difficulty at the outset which will long throw doubt upon the efficacy of the attempts which may be made to prevent air entering the veins during operations. The difficulty is, that no one can tell beforehand if the phenomenon will take place or not, supposing that such or such a vein is opened. Thus, suppose some particular precaution is taken in the case of three, six, ten, or fifteen patients, in whom tumours are extirpated near the apex of the chest, and that the air does not enter the veins in any one of them. Those who put their trust in the given precaution will not fail to conclude, that if nothing had been done some of the patients would have sunk. Yet this conclusion, which would seem very natural, might be altogether erroneous; for the cases quoted of the introduction of air into the veins are very rare, compared with the number of operations of a kind which would seem to favour it. I have myself more than fifty times extirpated submaxillary, parotid, axillary, supra-clavicular, or supra-sternal tumours, obliging me to go very near the great veins of those regions, and often even to open them; yet the case which I related above, and one of a young boy, are the only ones which alarmed me, even for an instant, as to the introduction of air into the veins. I will add, that when assisting M. Roux, I saw him make a large incision in the upper part of the subclavian vein in one case, in the upper part of the axillary vein in another, and in the lower part of the internal jugular in a third; and yet nothing which could be referred to the introduction of air ever resulted. When we reflect on the number of amputations which have been performed at the shoulder-joint—on the frequency of operations in the axilla for cancerous or lymphatic tumours—on the numerous instances in which arteries have been tied above the clavicle, and in the carotid regions, without any results comparable to what takes place when air enters the heart—we are forced to admit, at least, that the accident in question is hard to happen. How then,

can we be certain that, if it does not take place in a given instance, we are to attribute this to our precautions, rather than to the natural resistance of the frame? Hence we see that, to settle the question of preventive means, requires, like the rest of the subject, numerous experiments. Let us, however, examine those which seem to have attracted attention up to the present time.

Compression of the thorax during the whole of the operation seems to me to be suitable neither in theory, nor according to the experiments already tried. If the compression were sufficiently strong to prevent any raising of the ribs, the patient would obviously suffer a constraint so considerable as to be insupportable. Moreover, the chest would be equally extended in its vertical diameter by the depression of the diaphragm: nor has it yet been shown that the dilatation of the heart has no share in the absorption of the air.

M. Poiseuille, reasoning as if inspiration alone could draw air into the heart (Gaz. Méd. 1837, p. 671), thought that it would be sufficient to prevent this accident, if we could make the patient carefully avoid deep inspiration. This recommendation seems to me to be connected with the same error which suggested compression to M. Gerdy. In fact, experience has long shown surgeons that, during serious operations, the chest of the patient is generally contracted spasmodically—that respiration becomes slow, and is performed by small movements—and that one has rather to excite patients to breathe deeply, than to prevent them from doing so, if we wish the venous circulation not to be too much clogged.

Compression of the veins between the heart and the incision.—When the idea of the introduction of air into the heart first arose among practitioners, the first remedy which necessarily presented itself was this kind of compression; it seems so natural and so certain. Thus Larrey says that, when bleeding in the neck, we ought to compress the vein below the incision until the bandage has been put on, if we wish to prevent air from reaching the heart, (Clin. Chir. t. i. p. 357.) Dupuytren does not omit to point out the same means when narrating his case (Arch. Gén. de Méd. t. v. p. 438), which he had previously communicated to the Academy. A similar recommendation is to be found in Mr. Barlow's memoir, (Gaz. Méd. 1831, p. 355.) M. Putegnat likewise gives it in his thesis. Dr. Warren states, at page 266 of his treatise on Tumours, that the surgeon who operates in the vicinity of the jugular, subclavian, axillary, or iliac veins, or even of the saphena, when it is dilated, ought to leave to the last the separation of the pedicle of the tumours on that side, in order to be better able to compress the veins before opening them. He adds that, if possible, one ought to compress the veins between the incisions and the heart. I have myself thought it necessary to insist upon this recommendation, and to follow it in some cases of extirpation of parotid and submaxillary tumours; but it must be confessed that the remedy is a weak one. If, like the first observers, we admitted the possibility of air being inspired at a very great distance from the heart, the remedy would deserve our serious consideration. In case of the internal saphena, or femoral veins, the iliacs, the veins of the arm, the face, and the external jugular, nothing would be so easy as to put it into practice; but if it is true that the introduction of air is impossible in all these regions, compression is useless as a preventive. On the other hand, who does not see that, in the region above the clavicle, the upper part of the axilla, and the upper hyoidean region, where it would be useful, it is inapplicable? The subclavian vein, concealed under the clavicle or the sternum, and separated from the ribs by the root of the scaleni, lies in such a manner that its calibre cannot be obliterated through the skin. It is the same with the internal jugular vein below the larynx. To which we must add, that compressing this vein, when we are operating near the parotid region, puffs up the face, and increases the size of all the veins, which we are in danger of wounding.

For these various reasons we must admit that compression of the venous trunk between the wound and the heart is a resource of small importance, and rarely applicable.

Hence it follows, that in practice we cannot count upon the efficacy of any of the preventive means which have been hitherto mentioned, and that all which is in the surgeon's power may be reduced to these three points:—

1st. To avoid with the utmost care wounding the internal jugular or subclavian veins in operations.

2dly. In cases where he is obliged to go near these vessels, he is not to separate the pedicle of the tumour without previously compressing it between his fingers on the side of the heart, or passing a strong ligature around it.

3dly. He must avoid as much as possible all stretching, pulling, and separating the parts, lifting the arm, or throwing back the shoulder or the neck, when the bistoury is near the great veins at the apex of the chest.

Curative means.—If the art of healing possesses nothing capable of preventing the air from entering an open vein, we must confess with sorrow that it is still poorer in any method of expelling air from the heart. I greatly doubt that it can be of much use to place the patient on his side, as recommended by M. Forget, (Trans. Méd. t. x. p. 75.)

Compression of the thorax—the jerking compression by which Nysten thought he could expel the air from the auricle through the incision in the vein—is evidently useless, at any rate in human beings. Even in dogs, the air contained in the right ventricle cannot be thus driven out, and the right auricle gives up but very little of it. Who does not see that, in human beings, the thorax, which is far less flexible than in dogs, can never be flattened to such a degree as to react efficaciously upon the heart?

The most obvious method of all, that is to say, closing the incision in the vein, which has been already frequently adopted, offers, perhaps, as much danger on one hand as advantage on the other. While it puts a stop to the introduction of air into the wounded vessel, unfortunately it also obstructs the exit of the air which the contractions of the heart tend to expel; so that it cannot succeed unless, at the moment when it is applied, the air is not sufficient in quantity to cause death.

Drawing out the air by applying the mouth to the aperture of the vein can hardly ever be applicable. The least reflection, too, would suffice to show that it could not be of advantage.

Artificial respiration, either by means of tracheotomy, or by introducing a tube into the natural passage, were means tried by Dr. Warren, but seem to me to have no object.

The introduction of a saline fluid into another vein, which he also recommends, would only be adding to the danger of the patient.

There remains the plan of drawing out the air by a tube or syringe, introduced as far as the heart. But this suggestion, which arose from experiments on animals, should be rigorously proscribed in human beings, if I am not mistaken. It is plain that it could be adopted only when the internal jugular was wounded; and it is equally clear that, in order to introduce the tube, the vessel must be placed in the precise condition most favourable to the introduction of air. Moreover, during a serious operation, it would be requisite first to enquire whether the symptoms which had supervened really depended on the phenomenon in question; then to look for the aperture in the vein; then to ask the assistants for the tube prepared for this purpose; then to introduce it, and apply the syringe or the mouth to it. Now all this would necessarily require more time than intervenes before the death of the patient, if the accounts of these cases are correct. In short, we have really nothing to oppose to this occurrence.

This is a painful avowal, no doubt; but it is the truth. We are reduced, therefore, to the general remedies against syncope, unless there is any efficacy in bleeding, which was formerly proposed, and is still extolled, by MM. Boulay, Leblanc, &c. To avoid extending the veins in the neighbourhood of the chest during operations; to apply the finger to the wound, and to suspend the operation for a short time, when the supposed characteristic sound is heard; to keep the patient in the horizontal position, and stimulate him

with the vapours of ammonia, alcohol, or vinegar; to use friction to his body, and to throw water upon his face—such are, at present, the only means which reason and experience allow us to use without apprehension.

Art. II.—CAUTERISATION, AS A CURE FOR CROUP.

Dr. Peronneau de Besson used cauterisation in the treatment of all pharyngeal and laryngeal inflammations. M. Felix Hatin employed it successfully in four cases of croup. The following cases he has recorded in a late French journal,[1] in which cauterisation was applied for the treatment of croup at its commencement; where he describes the method of M. Peronneau, and the modifications which the want of his instruments led him to make.

On the 18th of November, 1836, M. Hatin was called to the house of M. Delacroix. His daughter, of about five years old, was seized with all the symptoms of incipient croup. He ordered immediately the application of leeches, and communicated to M. Delacroix his fears respecting the disease of his child, for he was aware from experience how frequently the perturbating plan employed in this case was unattended with success.

The child of one of M. Delacroix's friends, who had been affected with croup, was cured by the application of cauterisation only. This circumstance being made known to M. Hatin, he desired the physician who had performed this wonderful cure to be called in. A few hours after M. Peronneau and M. Hatin met; as the cauterisation which M. Peronneau proposed appeared innocent to M. Hatin, he saw no obstacle to its practice being permitted.

The child was seated on its father's knees, who with one hand held its arms, and with the other kept its head against his chest.

The operator placed himself before her, holding in his left hand an instrument[2] proper for keeping the mouth open and the tongue depressed, and in the right a long curved *porte-pierre*, as a sound, and armed with a cylinder of nitrate of silver projecting a few lines, and firmly fixed in.

The operator introduced his tongue-depresser (*abaisse-langue*), and, immediately, passed down the fauces his porte-pierre, and moved it rapidly over every part for a second or two. He then withdrew both instruments, so as to allow the patient to respire.

Some minutes after he made another cauterisation like the first.

The operation ended, the child only complained of a sensation of pricking and a slight desire to expectorate, which disappeared after a few moments. It was then between nine and ten o'clock in the evening. She passed the night quietly. Next morning the croup had become simply catarrhal, and gave occasion to no more apprehension. Inspection of the throat showed that the tonsils, the posterior paries of the pharynx, and all the other points accessible to view, were covered with a milk-white eschar.

This eschar dropped off the following days in flaps, leaving after it a vivid redness, which was, however, accompanied with so little pain as not to interfere materially with deglutition.

The child recovered quickly.

[1] Revue Médicale Française and Etrangère, Oct., 1837.

[2] This instrument, described from memory, is a quadrilateral plate of steel of at least a foot in length, of one or two lines in thickness, and about an inch in breadth, curved longitudinally to an obtuse angle, and divided into two parts, one which serves as handle, the other to depress the tongue. The latter, in order to keep the mouth open at the same time, presents on its superior surface, and near its edges, two projections of about an inch and a half in height, and separated by a hollow space, through which the *porte-pierre* may be passed.

M. Hatin proposes, that should the cauterisation of the pharynx not answer every purpose, the trachea itself may be cauterised [?]. Under this impression, he will have an instrument constructed, with the assistance of which he hopes to overcome the difficulties and remove the principal dangers of this operation; but we apprehend he will not find this easy of accomplishment.

The second case was the son of M. Imard, Director of the Hôpital de la Pitié, between nine and ten years of age, who was seized with the first symptoms of croup on the night of the 5th of February, 1837. The parents sent for M. Hatin at six o'clock in the morning.

Before putting in practice the operation, he was desirous of having the advice of M. Serres, Member of the Institute of France, and one of the most justly celebrated physicians of the age. They both agreed, and the operation commenced, which was performed as in the preceding case.

In the evening of the same day the croupy cough had disappeared. That which remained was loose. It preserved this character, its intensity diminishing for two or three days following; cure speedy.

The next case was the son of M. David, Bourtibourg street, Paris, aged eleven years, who had been seized during the day with a cough, the peculiar timbre of which had excited its mother's solicitude. M. Hatin proposed cauterisation; it was agreed to. As he had no instrument with him fit for the operation, he procured from a neighbouring apothecary a cylinder of nitrate of silver, which he fixed securely in a quill, and lessened the flexibility of the feathered portion of the quill, by binding it upon a small stick, which served as a handle for his extemporaneous *porte-pierre;* in this manner he performed the operation, using the index finger of his left hand to depress the tongue.

The last case was the daughter of a Madame Cordier, living in Paris. He proposed to cauterise her throat. She consented, and he performed the operation immediately, in a similar manner.

Art. III.—CLINICAL LECTURE ON TUMOURS OF THE JAWS.[1]

Delivered at St. Bartholomew's Hospital, by Wm. Lawrence, Esq., F. R. S., &c.

Mary Edwards, thirty-two years of age, a married woman, and mother of four healthy children, came into the hospital from the country, on account of a swelling occupying the alveolar margin, part of the palatine plate, and the external surface of the left superior maxillary bone. She has always enjoyed good health, but has suffered much, since the age of fifteen or sixteen, from toothache and gum-boils. The teeth have gradually decayed and broken off, leaving rotten stumps in the jaw; she has had four or five drawn; two remain in the upper jaw, where there are still several fangs worn to a level with the gum. The present tumour made its appearance two years since, when she considered it as the beginning of another gum-boil: however it slowly enlarged, without inflammation, suppuration, or pain.

The enlargement, which is about the size of a large walnut, begins at the socket of the lateral incisor, and extends to that of the first molaris: it passes upwards, nearly to the edge of the orbit, raising the lip and cheek, so as to constitute a conspicuous deformity; and it continues for a short distance on the palatine aspect of the bone. It has a dense and firm feel, resembling in these respects the gum itself. It has been, and is, quite free from pain, and will bear considerable pressure. The surface is covered by entire and sound mucous membrane, and presents some slight inequalities on the convexity of the tumour. The colour is a deep red, approaching to livid. There is no enlargement of absorbent glands, nor any other morbid affection. The swelling already interferes considerably with mastication and articulation;

[1] London Medical Gazette, April 21, 1838, p. 153.

it is increasing, and will become more troublesome: hence the patient wishes that it should be removed by operation, if it can be done safely. I have not hesitated to assure her that the operation is free from danger, and that there is little or no fear of relapse. The slow growth, the firm consistence, the absence of pain and of disease in the absorbent glands, satisfied me that the malady was not malignant, although I could not venture to determine its exact nature. The only circumstance that could be regarded with suspicion, was the indication of considerable vascularity afforded by the deep red colour.

The patient having been prepared by the usual preliminary measures in respect to diet and the state of the bowels, as well as by removing the two remaining front teeth in the upper jaw, and two or three stumps, underwent the operation six days ago. A perpendicular incision was carried from the edge of the orbit through the entire thickness of the cheek and upper lip, terminating near the angle of the mouth. The lateral flaps formed by this incision were quickly dissected off, so as to expose the tumour completely. An incision was next made through the gum and palatine membrane at the base of the disease, and then a groove was formed, by means of a small saw, in the alveolar process at the front of the swelling. This enabled me to insert the blades of a cutting forceps, and to carry the instrument along the base of the swelling, which was thus easily loosened and detached. It came away as a distinct round mass, leaving a superficial depression in the alveolar aspect and external surface of the bone, of which the substance was perfectly healthy, both in this situation and where it had been cut through by the forceps. In the excavation which had lodged the morbid growth, the bone was bare and rough, from the pressure which it had undergone, and it exhibited a little increase of vascularity. There was free bleeding from several arteries, particularly the coronary and the trunk of the infra-orbital, rendering the patient faint: the coronary was secured by ligature. The wound of the lip was united by a hare-lip pin and twisted suture, applied near the edge of the red portion; a single suture was placed an inch higher up, and the intermediate portion was brought together by a small strip of adhesive plaster. The part was covered with a soft doubled rag, kept wet by dipping in cold water. In twenty-four hours after the operation the suture was cut out, and in forty-eight hours the hare-lip pin was removed, the union of the wound by adhesion being complete throughout. The wounded surface of the cheek has become united to the excavation in the bone, so that the latter is no longer visible. The patient is now, on the sixth day, sitting up in her bed well, and quite able to return to her home, in Sussex, which she means to do in a day or two.

The tumour, which I now show you, is not clearly referable to any of the various growths which have been designated by that name. It does not originate from the bone, nor is it a change of structure in the gum. It is not one of the compact fibrous productions intermixed with osseous spiculæ or laminæ, originating from a bone, and constituting an innocent species of osteo-sarcoma; it is not one of the vascular fungous growths from the gum, nor one of the compact fibro-cartilaginous productions arising from a change of structure in the gum and palatine membrane; it does not present, in any degree, the consistence and other characters of medullary growths. It is a well-defined tumour; close, compact, and homogeneous in texture, presenting a mottled appearance on a section, inasmuch as it is generally red, with an intermixture of paler portions; not otherwise connected with the bone than by being partially imbedded in a hollow on its surface, and adhering closely below to the gum and palatine membrane. It seems to me a new formation or tumour, in the proper sense of that term, approximating in closeness and compactness of structure, as a morbid production, to the natural organisation of the gum. I see in it no evidence of malignant character, and therefore expect that the cure will be permanent. It would come under the technical denomination of epulis, which includes morbid growths from the gum, or others closely connected with it.

Some years ago I removed a mass, twice as large as the present, from the alveolar edge of the left upper jaw-bone, in a strong healthy woman, fifty years of age. The disease was of six years' duration, and considered by the patient to have originated from a decayed tooth; it had increased slowly, and without pain. It was a large mass, extending from the alveolus of the canine tooth to the back of the upper jaw, where the two last grinders were imbedded in it, filling one side of the mouth, and causing so much annoyance by its interference with articulation and mastication, that the patient was very desirous to have it removed. This growth was of cartilaginous firmness, of pale colour, like that of the palate, and with irregularities of surface. Its limits could not be ascertained accurately towards the cheek and back of the mouth. The imbedded teeth were extracted before the operation. An incision, commencing a little above the angle of the mouth, was carried through the upper lip and cheek to the lower edge of the zygoma; it was slightly curved, with the convexity upwards, to avoid the parotid duct. Several arteries bled freely, and three were tied; but a large quantity of blood was lost, not less than between one and two pints. The base of the growth was then laid bare on the outer side, and a slight groove was made in the same situation by a small saw. The tumour was now readily detached with the cutting forceps, the antrum not being exposed. The wound was united by three sutures, and by a hare-lip pin placed at the angle of the mouth; and the parts were covered by a damp cloth. The passage of blood into the throat, from vessels seated too deeply to admit of ligature, was a source of considerable inconvenience in the operation, by exciting efforts to vomit. In the course of the evening free bleeding came on, and the blood again passed into the throat. It was necessary to open the wound; a large vomiting of blood took place, and the patient was supposed to have lost twice as much blood as she did in the operation. The wound was again brought together as before. It healed throughout by adhesion, and recovery was complete in about a week, the left side of the jaw merely presenting the same appearance as in those who have lost their teeth. This tumour was of close, compact, fibro-cartilaginous structure, at least equal in density to the substance of the gum. There were spiculæ of bone at its basis, and it was necessary to smooth off two or three bony irregularities after the mass had been detached.

[We may conclude that there has been no return of disease in these cases, as both patients were addressed to me by persons whom I knew, and they promised to return to the hospital, or to let me know, if there should be any fresh growth.]

The jaws are so frequently swollen from disease of the bones, or of parts immediately connected with them, and these affections are so various in their nature and treatment, that any contribution calculated to elucidate the latter points is of some value. I shall therefore mention to you shortly two cases which have recently come under my observation:—

A healthy female, between thirty and forty, came to the hospital for my opinion respecting a swelling in her lower jaw. In size and shape it might be compared to the half of a large walnut; it was seated on the outer surface of the lower jaw, and inseparably connected to the bone. Examined through the skin it appeared firm and solid; it was readily felt through the membrane of the mouth, just below the gum; and here fluctuation was perceptible in it. It had been growing for more than a year, and was unattended with pain. The corresponding portion of the gum contained the fangs of a molar tooth, which had been lost by gradual decay. I punctured the part from the mouth, and let out about a dessert-spoonful of fluid, of watery consistence, containing the minute glistening particles which are not unfrequently seen in the fluid of hydrocele. Nothing unhealthy could be discovered in the bone. After a few days the opening was found closed, and the swelling reproduced; it now contained a thickish bloody fluid. I exposed the cavity more largely, and had the decayed stumps removed. In a little time the puncture was closed, and the tumour had disappeared.

Soon after I saw a similar but larger swelling in the upper jaw of a middle-aged woman. It occupied the excavation of the bone below the orbit, causing a conspicuous swelling, and projected a little at the base of the gum; there was manifest fluctuation. The decayed fangs of a grinding tooth had the same relation to the swelling as in the former instance. The complaint had existed for three or four years, without causing pain; and the patient was now induced to seek assistance by the swelling, which was increasing. The case was treated like the last, with the same result. The fluid contained in the swelling was similar to that in the two other cases.

[A third case has lately been seen at the hospital, corresponding with the two preceding, except that no carious teeth were present, though some had been extracted shortly before the swelling was noticed. It was in the upper jaw of a man aged about thirty, and had existed about two years. About a tablespoonful of fluid escaped when the cyst was punctured. A probe introduced at the opening penetrated so far, and could be moved so freely, that there was no doubt of its having entered the antrum, although fluid subsequently injected has not passed into the nose. A portion of lint was introduced to maintain the opening, from which a puriform discharge to some extent has occurred. The lint was left out at the end of three or four weeks, and a solution of sulphate of zinc was injected; but the opening has not yet closed, although eight or ten weeks have elapsed since the puncture.]

Four or five years ago I saw a boy, about fifteen years of age, with a swelling of the right upper jaw-bone, apparently arising from a general enlargement of the antrum. The affection had come on gradually, and was unattended with pain. The enlargement of the bone caused swelling of the cheek, and unnatural projection in the mouth above the alveolar process. The prominent part was a little irregular on the surface, and the projections yielded a little to pressure, so as to make it probable that there might be fluid within. The palatine plate was depressed. There was no disease of the soft parts. I punctured the swelling in the mouth, and its contents escaped in the form of a thin mucous fluid. I then cut out a thin slip of the swelling with the cutting forceps, so as to expose the cavity freely. Nothing unhealthy was observed in the lining of the antrum. The opening slowly closed, the swelling subsided, and no further inconvenience has been experienced from this disease, which seemed to be simple enlargement of the antrum from increase of its natural secretion.

I beg to direct your attention to the means employed for uniting the wound, in the case of Mary Edwards, and to their completely successful effect. You have had occasion to witness a similar proceeding and result in the case of the female whose breast I removed for carcinomatous disease three weeks ago. Although it was necessary to remove the entire mammary gland, the integuments were perfectly healthy, and were saved in sufficient quantity to close the wound, which was about eight inches in length. It was brought together by sutures and then covered with a wetted cloth. The sutures were cut out in twenty-four hours, and the incision was united by adhesion in its whole length, so completely, that it was necessary to open it a little three days afterwards, to let out some bloody purulent fluid collected in the tract of the ligatures. At the end of a week this patient was sitting up in bed quite well, and could then have traveled safely to her residence in the country, fifty miles distant; but her friends did not come for her till three or four days after.

The uniting of wounds with sutures is not in favour with modern surgical authorities, who represent it as at least unnecessarily painful, and often directly injurious by causing irritation, and consequent inflammation and suppuration of the wound. From some of the representations on this subject, it might be inferred that the use of sutures in wounds would prove a surgeon to be unacquainted with the principles of his profession. I consider these views to be entirely incorrect, and that the union of wounds by suture is much better calculated to secure adhesion than the employment of

adhesive plasters. The objection to the use of sutures formerly employed, are applicable to the mode and not to the principle of proceeding. Large needles were used, and thick threads; these were carried deeply, with the object of uniting the wound throughout; and they were left in until they excited inflammation, swelling, and suppuration, and often cut through the parts embraced. We should employ small needles with sharp points and edges, and slender silk threads, such as we use for tying arteries: we should include teguments alone, or in conjunction with the subjacent adipose texture, and not attempt, in general, to unite muscular parts; and, which is a point of particular importance, we should cut the sutures out at the end of twenty-four, or at farthest, of forty-eight hours. The edges of the wound are sufficiently agglutinated to hold together before the earliest of these periods: if, however, union by adhesion has not occurred before that time, it certainly will not take place afterwards, and the sutures consequently can be of no farther use. When employed in this manner, sutures are sometimes necessary, often advantageous, and never hurtful. In this mode of proceeding the wound and the adjacent parts are left uncovered, so that they can be kept cool by exposure to the air, or by the application of wetted cloths.

The adhesive plasters commonly employed are irritating, and capable of causing heat and redness in the sound skin of a healthy person; how much more likely are they to irritate a wound, where the parts may be expected to inflame in consequence of the operation. When a considerable wound, and the neighbouring surface, to the extent of some inches, are thickly covered with such plasters, with the addition of external dressings and bandages, we may reasonably expect that the adhesive process will fail—that inflammation and suppuration will ensue. The local mischief and suffering are not the worst part of the evil under such circumstances. The inflamed wound disturbs sympathetically the alimentary canal, disorders the circulation and the secretions, thus inducing a constitutional disturbance which is appropriately denominated *traumatic* fever; this reacts on the wound, exciting and maintaining spreading inflammation of the integuments.

It is no slight argument in favour of sutures that we use them on most occasions when it is important to obtain accurate adjustment of the divided parts and speedy union by adhesion. The operation for hare-lip, those for the removal of the upper and lower jaw, and wounds of the face, are examples. The best specimens that I have seen of healing by the first intention have been in wounds treated by sutures in the manner already described, of which I could cite to you numerous and various instances. In a case where I removed the upper jaw, the disease having been attended with considerable enlargement, so that extensive incisions were required, the flaps were united by three or four hare-lip pins and several sutures. The latter were all cut out in twenty-four hours, and the former were removed at the end of forty-eight hours. Union by adhesion was perfect throughout, except in a small part of the wound near the external angle of the eye, where I had trusted to adhesive plaster; at this part the skin was red, and the wound suppurated.

Case of sloughing chancre in a young female.—The cases which I mentioned to you in a former lecture, of sloughing, as the primary effect of the venereal poison, were all in males. We have now in the hospital a girl of sixteen, whose case is interesting in reference to the natural history of syphilis, showing that the venereal poison may destroy the vitality of the part to which it is applied, without exciting surrounding disturbance or disordering health. Sarah Woodruff, in Patience ward, was servant in a family in Goswell street. She went out in the evening with two females older than herself, and passed the night with a man, with whom she represents that she had sexual intercourse for the first time. This happened a few days before she came to the hospital. On her admission she had a slough at the right side of the entrance of the vagina; its longest axis was nearly an inch. There was no material inflammation or swelling of the

surrounding parts, and little or no pain. The slough was bounded by a line, at which the process of separation had hardly begun. The appetite and sleep were unimpaired, the circulation undisturbed, and the girl appeared in perfect health. She was confined to bed; the part was poulticed; and no medicine administered, except an occasional aperient. The separation took place favourably, the depth of the mortification being at least one third of an inch, and the surface healed rapidly, a little discharge from the vagina remaining after the cicatrix had been completed. The patient is now ready to leave the hospital. The story told by this female was corroborated by the state of her sexual organs, which were healthy except at the mortified part. The gangrenous affection was not referable to irritation from excessive coition, nor to any constitutional unhealthiness; it must be regarded, therefore, as a local effect produced by the application of an animal poison. The influence of the virus seems to be exhausted in the destruction of the part; the subsequent processes of granulation and cicatrisation being performed as healthily as in the case of slough caused by any other agency. The symptoms are more severe when this affection occurs in the male, where the suffering seems to arise principally from the pressure of the inflamed prepuce on the inflamed glans, and is immediately relieved by dividing the prepuce. This aggravation of the mischief does not occur in the female, where the parts are free from all pressure. The cases formerly related show, that in the male as well as in the female, the operation of the virus is arrested by the occurrence of mortification; the dead part is cast off, and no further noxious agency is observed.

BIBLIOGRAPHICAL NOTICES.

American Journal of Pharmacy.[1]

This valuable periodical, which has received commendations both at home and abroad, and which has recently been conducted—and ably conducted—by Dr. Carson, Professor of Materia Medica and Pharmacy in the Philadelphia College of Pharmacy, has just commenced its tenth volume,—the fourth volume of the new series. Dr. Bridges has been associated in the editorship.

It consists of three departments,—1. Original Communications; 2. Selected Articles; and 3. Miscellany. The original communications are the following:—1. Boullay's Filter and System of Displacement, with observations drawn from experience. By Augustine Duhamel. 2. Observations on some of the Camphoriferous Essential Oils, and on the Resins evolved from some of the Volatile Oils by their reaction with Sulphuric Acid. By William Procter, Jr. 3. Notes on Falsifications and Adulterations. And 4. Notice of the true Jalap Plant.

The journal is worthy of special patronage, and we recommend it strongly to both the physician and the *pharmacien*. It is published quarterly.

[1] The American Journal of Pharmacy, published by authority of the Philadelphia College of Pharmacy. Edited by Joseph Carson, M. D., Professor of Materia Medica and Pharmacy in the College, and Robert Bridges, M. D. Assisted by a publishing committee, consisting of Daniel B. Smith, Charles Ellis, Prof. Wood, Prof. Bache, Dillwyn Parrish, J. C. Allen, W. Hodgson, Jr., and Elias Durand; correspondents, C. Adamson, and O. Hull, New York; Prof. R. E. Griffith, Charlotteville, Va.; Prof. W. R. Fisher, and G. W. Andrews, Baltimore; Prof. Bigelow, Boston; and R. Peter, M. D., Lexington. Vol. X. New Series, Vol. IV., No. 1, April, 1838. 8vo, pp. 88. Philadelphia.

Coulson on Diseases of the Bladder.[1]

The work of Mr. Coulson—one of the most respectable of the London surgeons—which is reprinted in the present number of the "Library"—will be in the hands of our most distant subscribers in a little more than a fortnight after its reception in this country; and such was the case with the work of Syme on the Diseases of the Rectum, published in our last number but one. Nothing can more signally exhibit the value of such publications as the "American Medical Library" than the fact, that in the compass of not much more than one of our numbers, constituting only one twenty-fourth part of the year, we have been able to place before our readers three works which in England are sold for seventeen shillings sterling, and which could not be retailed here for less than six and a half or seven dollars. These three works do not actually cost in the "Library" more than half a dollar.

The chapters in Mr. Coulson's work are ten in number, embracing respectively,—Irritability of the Bladder; Paralysis of the Bladder; Acute Inflammation of the Mucous Membrane; Sub-acute Inflammation of the Mucous Membrane; Acute Inflammation of the Muscular Coat; Chronic Inflammation of the Muscular Coat; Inflammation of the Peritoneal Coat, and of the surrounding Cellular Tissues; Fungus Hæmatodes and Cancer of the Bladder; Foreign Bodies in the Bladder, and Operation for Stone; and Wounds and Injuries of the Bladder.

Solidification of Carbonic Acid, by Dr. Mitchell.—Dr. J. K. Mitchell, of this city, has perfectly succeeded with this experiment. It consists in subjecting the gas in an appropriate apparatus to a considerable pressure, say to one of forty atmospheres. By opening a stopcock, the pressure is suddenly removed, and so great a cold is produced, that the gas, having become liquefied by the pressure, freezes, and escapes in the form of a beautiful white solid; the temperature of which is about —80° of Fahrenheit. In some experiments, performed in Edinburgh in February last,[2] the temperature during the process, it was supposed—but it was a mere supposition, for it was not tested—might have been as low as —180°. On mixing the solid carbonic acid with ether, Dr. Mitchell found that the temperature fell to —90°, the lowest point which he had noticed in any of his experiments,—a temperature, by the way, only 20° below that observed by Captain Franklin, on one occasion, in the northern part of this continent, in the open air.

The results of Dr. Mitchell's experiments, which we have more than once had the gratification to witness, will doubtless be published by that accomplished chemist. By means of an admirably constructed apparatus, he appears to have succeeded better than any of his predecessors or contemporaries in the experiment.

Detmold on the Division of the Tendo Achillis in Club-Foot.—In the last volume of the "Intelligencer,"[3] two communications were published, describing the history, and mode of performing, this successful operation for the removal of a distressing deformity. Recently, Dr. W. Detmold, of

[1] On Diseases of the Bladder. By William Coulson, Surgeon. 12mo, pp. 153. London, 1838.

[2] Edinburgh Medical and Surgical Journal, for April, 1838.

[3] See No. 13, for Oct. 1, 1837, p. 233, and for March 1, 1838, p. 418.

New York,[1] has detailed some cases of this nature operated upon by him, and in the remarks of the editor of the journal in which they appeared, appended to Dr. Detmold's paper, it is stated, that "the merit of being the first to perform the operation in this country is, we believe, justly due to our correspondent."[2]

We attach but little importance to the fact of precedency as to the adoption of any particular plan of treatment successfully practised elsewhere; but as a matter of history we doubt whether the merit of being the first to perform the operation in this country rests where the editor of the American Journal has placed it. Dr. N. R. Smith, of Baltimore, carried the operation into effect a considerable time, we believe, before the date of Dr. Detmold's cases. If we are in error, Dr. Smith will place us right; and we hope he will communicate the results of his experience on the subject to the profession.

Pennsylvania Hospital.—Dr. Thomas Stewardson has been recently elected one of the attending physicians to this institution in the place of Dr. William Rush, resigned.

Philadelphia Hospital, Blockley.—Dr. Robley Dunglison has been appointed one of the attending physicians to this institution, in the place of Dr. Stewardson, resigned.

NECROLOGY.

G. R. Treviranus.—This distinguished physiologist—author of many valuable contributions to science, and the co-editor of Tiedemann in the Zeitschrift für Physiologie,—died on the 16th of February, 1837, at the age of 61.

Louyer-Villermay.—This gentleman, who was a member of the Académie, died on the 22d of December, 1837, of apoplexy. He was the author of a treatise on Nervous Diseases (vol. 2, Paris, 1832); of many articles in the Dictionnaire des Sciences Médicales, and of various medical communications in the journals. His funeral *éloge* was pronounced by M. Pariset.

BOOKS RECEIVED.

From the Author.—The Structure of the Eye, with reference to Natural Theology. By William Clay Wallace, Oculist to the New York Institution for the Blind, and to the Orphan Asylum: late Physician to the Northern Dispensary, &c. 12mo, pp. 52. New York, 1836.

From the Author.—Fiske Fund Prize Dissertations of the Rhode Island Medical Society. Cholera Infantum, its Causes and Treatment. By David King, Jr., M. D. (with a motto.) 8vo, pp. 22. Boston, 1837.

From the Hon. J. Jackson, Member of Congress from Georgia.—Designs Nos. 1 and 2 for a Marine Hospital on the Western Waters;—the one to accommodate one hundred patients, the other fifty. Washington, 1838.

Die Krankheiten des Fœtus, von Dr. J. Grätzer, ausübendem Arzte und Geburtshelfer. 8vo, s. 272. Breslau, 1837.

[1] American Journal of the Medical Sciences, for May, 1838, p. 105.
[2] Ibid, p. 127.